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CCIII.

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TENSILE TESTS OF CEMENT, AND AN APPLIANCE FOR MORE ACCURATE DETERMINATIONS.

BY D. J. WHITTEMORE, Member of the Society.

READ AT THE TWELFTH ANNUAL CONVENTION, MAY 25TH, 1880.

With Discussion by WILLIAM P. SHINN, M. CORYELL, ASHBEL WELCH, JAMES B. FRANCIS, JAMES B. EADS, J. H. HARLOW, MAX E. SCHMIDT, E, S. CHESBROUGH, WILLIAM R. HUTTON, F. O. NORTON and D. J. WHITTEMORE.

For the purpose of ascertaining tensile strength, Lipowitz of Germany, in his work on Portland Cement, advises the use of briquettes having a form and size of breaking section 3 x 6 centimetres.

I understand that the area of rupture used by French Engineers is commonly 4 x 4 centimetres, though an area of 3 x 4 centimetres is sometimes employed.

In England the usual size is $1\frac{1}{2}$ x $1\frac{1}{2}$ inches. This size is extensively used by Engineers in this country. Sometimes the form is changed, keeping the area the same. Manufacturers of hydraulic cements in this country are using largely a breaking area of 1 inch square.

Many of the published statements we have, give the tensile strength per square unit of measure, omitting to mention the actual size of the briquette.

One of the objects of this paper is to show the importance of knowing the actual linear measure of the breaking section, and to consider whether, in comparison of results determined from briquettes of different sizes, some more satisfactory and correct unit of measure can be employed than the expression of area now so universally used. It is known to many experimenters that, in tensile tests particularly, the apparent strength per square unit of ruptured area is greater in small than in large briquettes, and in an endeavor to determine the ratio of this apparent variation I have made certain tests, in which investigation I employed cylindrical briquettes of two sizes, the larger one having a breaking section of two square inches, and a form and clutch appliance similar to that shown in my notes on Alfred Noble's paper of last year.* The smaller briquette and clutch is shown by plate herewith annexed. The breaking area of this briquette is one-half of one square inch, and its form and clutch, in my opinion, possess marked features of excellence. (See Plate XVII.)

It will be seen that each head of this briquette has its principal enlargement at three equi-distant places upon its circumference—each enlargement comprising an arc of about 58 degrees. The lower half of the face of these projections forms part of the surface of a sphere, the center of which is upon the axis of the briquette. The gimbal clutch has slots cut from the intrados of the inner gemel-ring of a size sufficient to allow the head of the briquette to pass through. The balance of the intrados is truly turned to fit the surfaces of the briquette enlargement, and when the briquette is inserted a proper distance and revolved about 60 degrees a secure and perfectly fitting seat is provided upon the inner gemel-ring.

To secure uniformity in moulding specimens I have designed a press by which, after the flask is charged with mortar, plungers act upon each end of the mass with some definite pressure. The annexed sketch shows this device, and needs no further description to understand its operation. (See Plate XVIII.)

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* Experiments with Appliances for Testing Cement, by Alfred Noble, Vol. IX., p. 186. Discussion by D. J. Whittemore, p. 199 (May, 1880).

The uniformity in tests by this arrangement of moulding and application of gimbals to the clutch is so remarkable that I will here state the first series of tests made with its use.

The mortar was made of neat cement, which was first mixed thoroughly, so as to be quite certain that any part should be an exponent of the whole. All samples were brought to the same degree of plasticity, and moulded under the same pressure, and when set removed from the flask and placed in water for a period of seven days—then tested, with the following results;

TABLE No. 1.

No. of Test.	Tensile Strength of Briquette.	Strength per Square Inch.	Per Cent. Variation from Mean.
	Lbs.	Lbs.	
1	74.06	148.12	2.8
2	69.69	139.38	3.9
3	69.69	139.38	3.9
4	75.31	150.62	3.9
5	72.81	145.62	0.4
6	71.56	143.12	1.3
7	73.13	146.26	0.9
8	73.13	146.26	0.9
Average	72.42	144.84	2.25

The average variation from the mean here shown is only about one-quarter of that I usually obtain by the use of my modified form of the common briquette, and less than one-half that ordinarily obtained by the use of the circular briquette of two square inches breaking area and cup clutch. Believing that any instrument devised for purely scientific purposes should never be patented I freely offer the design to all who may now or hereafter desire to make use of it.

For the purpose of conducting the inquiry first alluded to I selected four of the typical brands of American hydraulic cements used in the northern, western and south-western portions of this country, also one

sample of English Portland. The American brands selected were Norton's Rosendale, Buffalo, Louisville and Milwaukee. It is not the purpose of this paper to permit any invidious comparisons to be made as to the quality of any known brand. I shall, therefore, designate these samples by numbers, not, however, consecutively in the order named above. The following table gives the analyses of these cements:

TABLE No. 2.

No. of Cement.	Carbonic Acid.	Lime.	Magnesia.	Silica.	Aluminum.	Oxide of Iron.	Alkalin Water. Loss, &c.
1.....	3.06	45.17	16.52	23.40	8.07	2.45	1.33
2.....	6.48	36.32	14.47	24.50	14.32	2.93	0.98
3.....	4.37	33.97	15.21	28.04	12.82	4.60	0.99
4.....	0.96	40.75	25.25	22.22	8.68	1.18	0.96
Portland....	0.45	63.44	1.34	22.98	6.88	3.80	1.11

The following table shows the weight of the cements as determined by the method described in my remarks on Mr. Maclay's paper (Transactions, Vol. VII, page 275).

Fineness is expressed in the per cent. rejected by sieve of 60 meshes to the linear inch, wire No. 36. The last column shows the amount of water required in terms of per cent. by weight of cement to make all mortars of the same degree of plasticity.

TABLE No. 3.

No. of Cement.	Weight, Grains per cubic inch.	Per Cent.. Rejected by No. 60 Sieve.	Water Used, Per Cent.
1.....	328.5	29.2	39.6
2.....	333.5	10.5	38.7
3.....	364.8	3.5	33.9
4.....	394.3	8.5	31.2
Portland...	453.0	16.3	24.2

Since American hydraulic cements vary 20 per cent. in weight, it is almost impossible to devise any expression, referring to weight alone, that will determine what quantity of water is required to produce mortars of a uniform degree of plasticity. I have also failed as yet to determine any exact relation from bulk measurement for this purpose. Undoubtedly, the quantity of water required is somewhat dependent upon the fineness of the cement used.

The amount of water used was sufficient to render the mortar plastic under a vigorous use of the hoe, producing what may be termed over-stiff mason's mortar. Enough cement was mixed at one operation to make a briquette of each size. Four of each size and kind were so made, and subjected in the flask to the same pressure per unit of end area of briquette. When the mortars had set, each sample was immediately placed in water of a uniform temperature of 66° F., for a period of 7 days, and then tested.

The following table gives the average of four tests in each instance:

TABLE No. 4.

	Circular Briquette Breaking Area, 2 Square Inches.			Circular Briquette Breaking Area, ½ Square Inch.		
	1	2	3	4	5	6
No. of Mortor.	Total Tensile Strength. Lbs.	Tensile Strength per sq. inch. Lbs.	Tensile Strength per lin. inch of Circumference. Lbs.	Total Tensile Strength. Lbs.	Tensile Strength per sq. inch. Lbs.	Tensile Strength per lin. inch of Circumference. Lbs.
1.....	106.00	53 00	21.20	41.60	83.20	16.64
2	104.20	52.10	20.84	54.40	108.80	21.76
3.....	127.76	63.88	25.56	72.42	144.84	28.97
4.....	107.81	53.90	21.56	53.12	106.24	21.25
Average.....	.....	55.72	22.29	.....	110.77	22.15
Portland.....	619.20	309.60	123.84	292.70	585.40	117.08

From the above it is seen that the apparent strength per square inch of the smaller briquette is about double that of the larger. Taking the average of the aggregate of results from the American product, it appears almost exactly so. What measure of the breaking section of the two sizes of briquettes is, then, most nearly proportional to their strength? Not the area; but a glance will show that either the diameter or periphery of the breaking section is about proportional to the ultimate strength. The variation from this rule is shown in columns Nos. 3 and 6, in which the strength per linear inch of circumference is determined by dividing the total strength of each briquette by the periphery of the breaking section.

Casting about for some corroborative testimony, deduced from the determination of other experimenters, I find that John Grant, M. Inst. C. E., in his work "On the Strength of Cement," page 124, gives the results of an extended series of tests instituted for the purpose of determining the form and size of briquettes that should be adopted in conducting his tests. Neglecting his forms Nos. 1 and 2, which have no counterparts of different breaking areas, I give in the following table his results on Portland cement, with four forms, having a breaking area of $1\frac{1}{2} \times 1\frac{1}{2}$ inches, and four other almost similar forms having a breaking area of 2×2 inches. The results from similar forms, but of different sizes, I place upon the same line in the table.

The moulded samples were from the same cement and were kept in water for the time mentioned. The results given are the average of ten tests in each instance.

In the following table I have introduced the column representing the strength per linear inch of circumscribing line of breaking section of each briquette, and the amounts so ascertained approximate closely to each other, while the strength per square inch varies about as 4 is to 3.

Taking the average of all the determinations at 7 and 30 days of peripheric strength, and we have for the $1\frac{1}{2} \times 1\frac{1}{2}$ inches section 161.6 lbs. per linear inch, and for the 2×2 inches section 161.7 lbs. per linear inch.

This relation of equal peripheric strength is not mentioned in Grant's work. The evidence of its truth, however, is so strong when applied to tests of samples of the age mentioned, and varying in size from $\frac{1}{2}$ of one square inch up to 4 square inches, that any expression of strength of cements per square unit of breaking area is of little value, unless such statement is accompanied by the dimensions of the briquette tested.

TABLE No. 5.

STRENGTH OF BRIQUETTES, $1\frac{1}{2} \times 1\frac{1}{2}$ INCHES.

No. of Form.	7 Days.			30 Days.		
	Total lbs.	Per sq. inch. Lbs.	Per lin. inch. Lbs.	Total lbs.	Per sq. inch. Lbs.	Lbs.
3	1064.3	473.0	177.4	1145.4	509.0	190.9
4	918.9	408.4	153.1	969.0	430.7	161.5
5	924.0	410.6	154.0	967.0	429.8	161.2
6	860.0	382.2	143.3	912.0	405.4	152.0
Average		418.5	156.9		443.7	166.4

STRENGTH OF BRIQUETTES, 2 x 2 INCHES.

No. of Form.	7 Days.			30 Days.		
	Total lbs.	Per sq. inch. Lbs.	Per lin. inch. Lbs.	Total lbs.	Per sq. inch. Lbs.	Per lin. inch. Lbs.
10	1286.3	321.6	160.8	1460.3	365.0	182.5
7	1199.2	300.0	149.9	1354.7	338.7	169.3
8	1172.6	293.0	146.6	1315.1	328.8	164.4
9	1190.5	297.6	148.8	1371.2	342.8	171.4
Average		303.0	151.5		343.8	171.9

In view of these results, it is but natural I should conceive it possible that the surface of the specimens had acquired a tenacity not extending throughout the entire mass, and that a surface hardening had taken

place through some process of crystallization, or by the absorption of carbonic acid, forming a sub-carbonate.

Having witnessed the remarkable effects of the so-called carbonization process in the manufacture of cement sewer-pipe, and having tested many samples of cement mortar that had been subjected to the process, I was prepared to believe that this same effect might be produced to some extent in ordinary water exposures.

The so-called process of carbonization is executed as follows:

In a chamber constructed of a double thickness of two-inch plank, the joints of which are tongued, grooved and painted so as to render the same nearly air tight, is placed the moulded pipe as soon as removed from the flasks. Outside the chamber is a furnace for burning charcoal, the exit pipe of which, for conveying the product of combustion, is brought to, and terminates in, the centre of said chamber. Over the fire of the furnace is a small boiler for generating steam, and the steam so generated is carried by an escape pipe into the pipe conveying the product of combustion to the carbonizing chamber. The size of the chamber in this instance is sufficient to conveniently store pipe produced from the use of from 13 000 to 15 000 lbs. of cement. When the charcoal is ignited the absorption takes place with sufficient vigor, if the supply of steam is properly regulated, to maintain combustion for a period of about six days, when the burning charcoal smothers and dies out from insufficient draft. The amount of charcoal consumed in this time is about $2\frac{8}{10}$ lbs. to 100 lbs. cement in the chamber. Making due allowance for other matter than carbon in charcoal, this would show that if all the carbonic acid was absorbed by the cement, a saturation of from 5 to 7 per cent. had taken place. If from any cause the supply of steam be not sufficient, and the surface of the cement samples become dry, the absorptive process is arrested, and any subsequent steaming will not observably restore it. Again, should the supply become excessive, so as to produce absolute wetness upon the floor of the chamber, absorption is also arrested and the fire goes out. So delicate is this matter of steam supply that an allowance must be made for the hydrometric conditions of the outside atmosphere. In other words, less steam is required in wet than in dry weather.

Crushing tests on pipe subjected to this process, and on pipe made from same moulds, same cements, and *not* so subjected, have been made, many of which tests I have observed myself, and believe to have been

fairly conducted. The first were subjected, during the first six days to the carbonizing process, and then stored in sheds along with the uncarbonized.

The following table gives the results of one series of such experiments, each result being an average of two tests :

TABLE No. 6.

Age.	Carbonized.	Not Carbonized.
15 days.	2 979 lbs.	2 083 lbs.
45 "	2 871 "	2 691 "
75 "	3 329 "	3 168 "
105 "	3 622 "	3 454 "
137 "	3 901 "	3 779 "

These determinations indicate simply a hastening of the indurating action. Briquettes of many different brands of cement that I have placed in the carbonizing chamber for a period of from 50 to 60 days have shown double the tensile strength of those kept in water for the same length of time. Others have shown no increase whatever, but generally there is a marked gain. With these facts before me, I conceived it possible that water might absorb carbonic acid from the atmosphere, and afterwards impart it to samples of cements immersed therein, resulting in a coating of the briquettes with a sub-carbonate of considerable strength. To determine this, I instituted the following tests :

Of the five samples of cement before alluded to there were moulded eight briquettes of each. Four of these were placed in open jars filled with water, and common water was used in the initial mixture. For the other four, distilled water was used both in mixture and exposure, the latter being in glass tubes hermetically sealed. *In one of these tubes was placed a thermometer.* The samples in the open jars gave off the usual amount of *laitance*, while those in the tubes gave off none, so far as the eye could determine, to the end. There was, however, an efflux in the form of crystals thickly studding the surface of the briquettes,

and of sufficient size and quality to permit of collection for analysis. They proved to be crystals of hydrate of lime.

Exposure to the atmosphere soon converted a thin scale of their surface to a carbonate.

At the expiration of 40 days all the samples were tested, and a cutting taken from off the broken section of those exposed in open jars for an analytical determination of the carbonic acid absorbed. The following tables gives the average of each of the four tests. The briquette used was that with the rupturing area of ½ of one square inch.

TABLE No. 7.

Kind of Cement.	Average Tensile Strength of Briquette, Hermetically Sealed.	Average Tensile Strength of Briquette, In Open Glass Jars.	Per Cent. Carbonic Acid Absorbed.
No. 1.....	178.20	169.45	0.10
2.....	216.00	158.81	0.0
3.....	204.76	169.28	.00
4.....	190.20	153.54	0.39
Portland.....	277.57	316.64	1.06

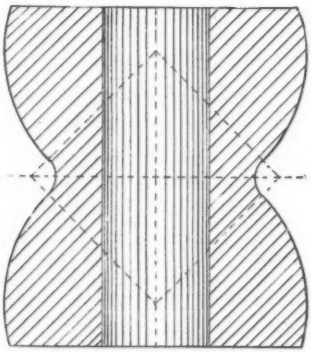
From the above showing, I do not feel assured that any of the American cements absorbed carbonic acid to any extent whatever, while the Portland did to the amount of about one per cent. The average greater strength of the American cements in sealed tubes, over those of the same kind in open jars, is about 21 per cent., but the reverse is the case with Portland, which undoubtedly absorbed some carbonic acid when exposed in water in open jars. The deduction I am forced to make is, that there is no appreciable formation of sub-carbonates upon the surface of American cements exposed in unsealed water at an age of 7 or even 40 days, but how can we account for the remarkable advance in strength of these cements that were enclosed in sealed tubes, over those that were not?

Perhaps the thermometer will inform us. The temperature of the water in the open jars subject to evaporating influences was quite uniformly 66° F., while that in the sealed tubes was from 3½ to 6

degrees higher, though all were kept in the same room. Under the influence of this greater temperature the Portland did not increase in strength, but fell behind those of the same kind in the open jars. Perhaps the slight carbonization that occurred in these more than counteracted the favorable elevation of temperature in the former.

In the light of these tests might it not be conceived that a portion of the remarkable effect of the co-called carbonization process alluded to in my notes on Mr. Maclay's paper, before referred to, might be attributed to the moderately high temperature necessary, in this process, to produce a highly charged hydrometric atmosphere in the carbonizing chamber?

Resuming the endeavor to determine whether the apparent peripheric strength really existed, I reasoned that this would be shown by testing briquettes of large peripheric dimensions inclosing greatly reduced areas. This was accomplished by inserting a core longitudinally through the centre of the larger circular briquette before mentioned, and the area of this core section was one-half of one square inch, thus producing a hollow briquette similar to that shown in the following sectional sketch, having a peripheric measurement inside and out 50 per cent. greater, and an area 25 per cent. less than the solid one of the same exterior dimensions.



Specimens were moulded under like conditions, of both the hollow and solid form, and also of the form of ½ square inch breaking area, kept in water the usual 7 days, and then tested with the following results :

Kind of Briquette.	Length of line bounding the breaking section, inches.	Area of Briquette, square inches.	Strength of Briquette, lbs.	Strength per Square inch, lbs.	Strength per Linear inch, lbs.
Hollow.	7.5	1.5	93.10	62.07	12.41
Solid	5.0	2.0	136.50	68.25	27.30
½ solid.....	2.5	0.5	67.50	135.00	27.00

These determinations indicate, if they indicate anything, that there is no great difference in the absolute strength of different portions of the mass subjected to water exposures for the time stated.

Cannot this anomaly of apparent peripheric strength be explained satisfactorily? It is known that mortars, like all other brittle substances, possess elasticity. In tensile tests the application of stress through the clutch is at the surface of the specimens.

The greater the elasticity of the substance under test, the more uniformly will the strains be distributed throughout the mass of the breaking section, but in brittle substances, like that we are now considering, the elastic limit is reached at the surface while great inequality of strain exists throughout the mass. Rupture must first occur at the surface, at once reducing the area to the limit at which complete separation takes place.

Tests of abrasion, or of impact, might have been resorted to in making this inquiry, but these methods afford no means of determining absolute tensile strength in pounds per square or linear unit of measure.

In conclusion, I desire to express my deduction by formula, the correctness of which I leave for further experiments and other experimenters to verify or disprove, when applied to briquettes varying in rupturing area from ½ to 4 square inches, and not exceeding the age stated in this paper.

Let P represent the periphery of breaking section of known strength, S per square unit. The strength S' per square unit of a briquette of similar form and material having a periphery P' will be

$$S' = \frac{P\,S}{P'}$$

However repugnant this may be to our usual practice, the logic of fact must govern in the end.

Discussion at the Twelfth Annual Convention upon the above paper No. CCIII., Tensile Tests of Cement, by D. J. Whittemore, and also upon Paper CXCVII., American Natural Cement, by F. O. Norton, Vol. IX., p. 278 July, 1880.

William P. Shinn.—I regard this subject of the tests of cement as one that has heretofore received but very little attention from the profession compared with its importance. The experiments of Mr. Whittemore are certainly of very great interest. I deduce from his paper that he has made no comparison of the comparative strength of briquettes of different sizes per square inch of breaking section for long periods of exposure, longer than seven days to forty, as I understood it, and it would be very interesting to know whether the same rule holds good for long periods of exposure, say from six to twelve months.

D. J. Whittemore.—My own tests extend to seven days. Grant's which are corroborative of mine, extend, one series, to seven days, and the other to thirty days, and the thirty days shows almost precisely the same as the seven days. We have no experiments of any experimenter within my knowledge, extending beyond that length of time.

M. Coryell.—There is one remark made by the gentleman, in his paper, stating that under atmospheric pressure the cement hardens very much. Many years ago I was connected with Mr. A. Welch in building the Chesapeake and Delaware locks, and he ordered certain specimens of cement to be tested. I did test those cements according to his direction, and I had to report to him that they would not come up to his requirements. The circumstances were such that we had to force the locks along, and we had to use the cement as we could get it, and the locks were completed and finished. But for some cause or other we had to take up a portion of the floor and examine the cement, and I reported to him that the cement was not at all satisfactory; still we had done the best we could. After a long while, and when the locks were about to go into use, Mr. Welch requested me to examine the locks at all these points that I thought might be weak and defective, to see whether the cement had hardened properly. I did so, and I had the satisfaction of finding that the cement had increased in hardness very much by being taken

from the exposure to the atmosphere and covered with water some six or eight inches deep, and all the tests I made in that lock bottom were found to be satisfactory. The cement assumed all the properties that it was intended to assume, and we have never heard of any disturbance or break in it since the completion of those locks.

ASHBEL WELCH.—I think it is very desirable that these experiments should be extended over long periods of time. We all remember in our experience, probably, occasions where cement has been rejected, where it did not stand the test, where after a few days it was found entirely unsatisfactory, and I remember one case where a cement barrel was tested and found to be defective, and it was tumbled into the river. We had occasion afterwards to remove that—some months afterwards—and the best way to remove it was found to be to blast it.

JAMES B. FRANCIS.—There is one other important matter in connection with this, and that is the temperature of the water with which it is mixed. It has great effect on the early hardening of the cement—the temperature of the water. Mixed with water of 32° it may be two or three days before it is at all hard; mixed with water of 70° or 80°, in a few hours it will be thoroughly set; and it seems to me that this is an important matter to be taken into account.

D. J. WHITTEMORE.—It was 66 degrees Fahrenheit; it didn't vary a degree except in the sealed jars; there it stood at the normal temperature of the room.

ASHBEL WELCH.—If you will allow me a moment to travel out of the record, I would mention that American cements were discovered about sixty years ago, by Mr. Canvass White. He was sent over by the State of New York, when they were about building the Erie Canal, to make provision for cement in England. He found there were two difficulties about getting and using that cement. In the first place, it was very dear. In the second place, after they got it here, after being six weeks on the ocean, they found that it was good for nothing. That put Mr. White upon his ingenuity, and at Chittenango, in western New York, he found or judged that cement rock consisted of a mixture of slate and lime-stone, and where these two formations met together he conjectured that the mixture might be hydraulic. He examined it and found it so, and that was the beginning of the history of American cement. The State of New York gave him $10,000 for his discovery.

JAMES B. EADS.—Before dismissing the present discussion, I

would suggest that there are few materials that are tested by engineers which give such a wide range of strength as cements. In my own experience, I have been struck with this great range. It is very different from that which we have in steel, iron and other materials, wood, stone, etc. But there is a degree of elasticity in the hardened cement which Mr. Whittemore speaks of as being a brittle material, that possibly may explain the phenomena of so much greater strength being developed by the small briquettes than the large ones. Cement admits of very considerable expansion before fracture, and very considerable shortening under pressure. I had occasion to make tests to determine this matter in connection with the St. Louis bridge, and some very curious results were developed by it. Among the materials found very much more elastic than originally supposed to be, was stone. For instance, we had a small column of stone, about an inch in diameter, magnesian lime-stone, and it shortened about a quarter of an inch. It was ten inches in length, and it shortened nearly a quarter of an inch under compression without fracture. Cement is much more elastic, and in that elasticity may be discovered the secret of this curious phenomenon of the greater strength of the small briquettes. If we apply the scientific uses of the imagination to the subject, and suppose that a briquette under test be only partially solid, and enlarged at each end, and undertake to extend it, we find a depression at each end showing that the interior particles are not strained to the extent which the surface is, and as the area increases with the square of the diameter, while the part which first feels the impulse of the tension only increases with the diameter, we see at once that the larger the briquette, the smaller would be the strength developed in the tests. I would add that I think the same phenomena would be found in testing iron and steel.

William P. Shinn.—It is to a certain extent.

James B. Eads.—The larger the sample under test, the smaller would be the strength per square inch.

James B. Francis.—As Portland cement has been referred to here frequently, I think it may be interesting to state that last summer I visited one of the large establishments where it is made, and perhaps a few minutes explanation in regard to it may interest some of the members present. It is made out of the material found on the spot. This establishment that I visited was on the bank of the Thames, a few miles below London, at North Fleet, where they are making a hundred or more

tons per day. The material is chalk found in a hill close by, and mud found in the river directly in front or near by, which was dredged out of the river as being the cheapest place to get it. That is all the material that is used except fuel, nothing else whatever. These are mixed in proper proportions which are got at by frequent testings in the way mentioned here, and the excellence of the cement depends on the completeness of the manipulation and close watching of the preparation. The first process, as I recollect, was a rough mixing of a proper quantity of chalk and clay, which were measured out in measures of a certain quantity of clay and a certain quantity of chalk, and ground very roughly, and after that passed through a stone like a grist mill, and the material was then allowed to settle and dry, with the waste from the kiln in which the material was afterwards burned. It was then cut into lumps and put in this kiln, with the fuel which consisted of waste small coal from the gas works, and burned at a very high temperature, so that in looking under the kiln I could see the melted material, where there was a little excessive heat, run off. It was at as high a temperature as they could burn it without melting. After the fuel was all burned out, the material in the kiln was taken out and broken in a Blake crusher, and was then ground in a mill stone, like a common stone, such as the millers use, and that was the end of the process. It was then packed in barrels or boxes and was transported. The price at the works was about $7 per ton of 2 240 pounds. It seems to be a very remarkable result to obtain from such simple materials in that way.

J. H. Harlow.—The main points of the paper which has been read have been in regard to using the tensile strain as the only requisite necessary in buying cement. Now they should use the others—the fineness and the activity—the fineness, because the finer the cement the more perfect the mixture would be with the sand added to it; and the activity, because the cement that cannot be used on this first set, is not as strong on the second as on the first. I made an experiment at the Government works at Pittsburgh, on the second sets of cement, and I found with one exception (a Portland cement manufactured near Pittsburgh), that they were from 50 to 75 per cent. weaker than on the first; that one special cement retained its strength. We had another cement there that we tested, and its activity was so quick in setting that at the end of ten minutes it was strong as most of the other cements at the end of 24 hours, while at the end of three months the strength of this particular cement

was only as strong as the others setting slower ; at the end of six months the slow setting cements were 50 per cent. stronger than the quick setting cement, and judging by all we observed on the stone work built there at the end of two or three years, this cement was just about as good as mud.

MAX E. SCHMIDT.—I would like to ask Mr. Norton whether he made any tests of transverse strength in cements, and compared them that far.

F. O. NORTON.—No.

MAX E. SCHMIDT.—These tests were made lately, and it was found that the foreign brand showed very much higher than the American brands tested. There were eight inches between supports, and the result was for the foreign brands about 40 pounds per square inch of transverse strength, while the American brands did not give over 20.

F. O. NORTON.—At what age were they broken ?

MAX E. SCHMIDT.—Seven days.

F. O. NORTON.—Seven days of Portland cement will show from three to five times as great strength as American cement. At seven days Portland cement has two-thirds of its maximum strength. Those curves (pointing to the diagram upon exhibition), are all carried along for a year, showing the ultimate strength of the two cements.

MAX. E. SCHMIDT.—The strength of the home brands was increased by having them immersed in water later than the foreign brands ; keeping them out 24 hours and then immersing them, and in that manner getting 50 per cent increased transverse strength.

E. S. CHESBROUGH.—I would like to make one suggestion to the Society. We have heard of a great many experiments with different kinds of cement, English and American. About twenty-five years ago I made a good many experiments by mixing different kinds of American cement, and, although the results generally were not satisfactory, and there was no improvement, it happened in one case that the best cement when mixed alone was not so good as when mixed with another cement that was rejected entirely and considered good for nothing. But the two mixed together made a better cement than the one which was better than all the others used alone. I regret very much that we did not at the time preserve an analysis and accurate records of the tests, but I simply throw out that suggestion to members of the Society to say if they have not tried it they may possibly obtain valuable results by mixing the cements of different kinds, and experiments of that sort. The

resulting cement that we obtained was really better than some of our lime-stone in hardness, after it had been kept some time, but at the great fire in Chicago the samples were all lost. As a matter of interest in this connection I will say that the inferior cement which had been rejected was supplied for a time, but in consequence of the carelessness of the manufacturer in letting the standard of it fall below what it was at first we finally had to reject it altogether, as it then proved to be an injury instead of a benefit.

D. J. WHITTEMORE.—I have made cement from twelve different layers of the cement ledge, and then any mixture I might make of those layers was an improvement over any individual layer used in the mixture. I am very careful that cement for any critical purpose should be thoroughly mixed. As far as the tests of Grant are referred to, it should be borne in mind that his tests are made on 1½ inches square, while Mr. Norton's, I understand, are on 1 inch square.

JOHN BOGART.—In the comparisons presented the tests are reduced to the same area; Grant's were on 1½ inches square; Mr. Norton's are actually 1 inch—these tables are for 1 inch.

D. J. WHITTEMORE.—And the variation if the rule holds good that I I have shown in my paper, would be as 4 is to 6. Mr. Grant has made tests with the Roman cement of England up to a period of five years, according to his last report, and he has extended his experiments with Portland to 7 or 10 years, perhaps. The Portland reaches its ultimate strength at about two years. The Roman cements of England continue to improve, as his showing makes it, up to a period of five years; or, as near as I can calculate or formulate it, the Portland improves at a ratio proportional to from the 8th to the 12th root of its age, and the Roman from the 4th to the 5th root, and the exhibit indicates that the American hydraulic cement indurates very much as the Roman cement of England does.

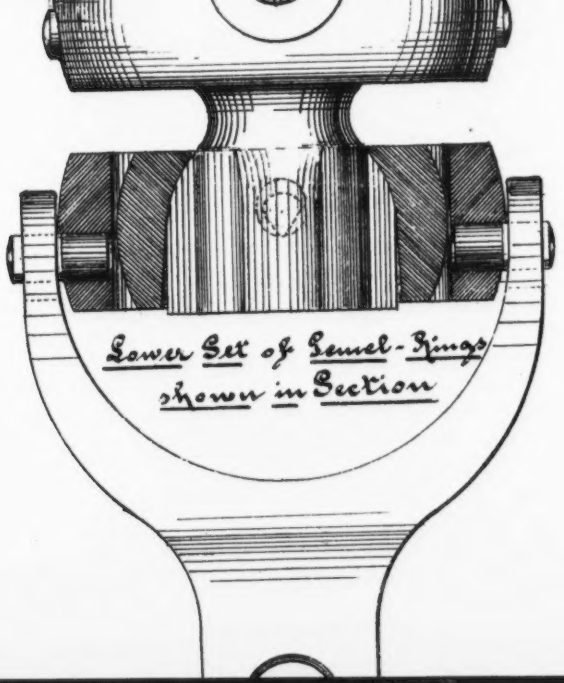
WILLIAM R. HUTTON.—I would ask Mr. Chesbrough if the two cements he mixed were both of the same degree of hydraulic activity?

E. S. CHESBROUGH.—They were not; one is what is called Clark's La Salle Cement of Illinois; the other was called at that time the Sandusky Cement of Sandusky. It had very little activity indeed; it was so inferior that it had ceased almost to be of value at that time; but the manufacturers, when they found it was adapted for mixing, were so elated with it that they became careless, and sent in a still more inferior

Plan of Briquette.

Side Elevation of Briquette

Briquette in Perspective.



Lower Set of Kemel-Rings shown in Section

Top-View of Inner Kemel-Ring.

Block of Briquette.

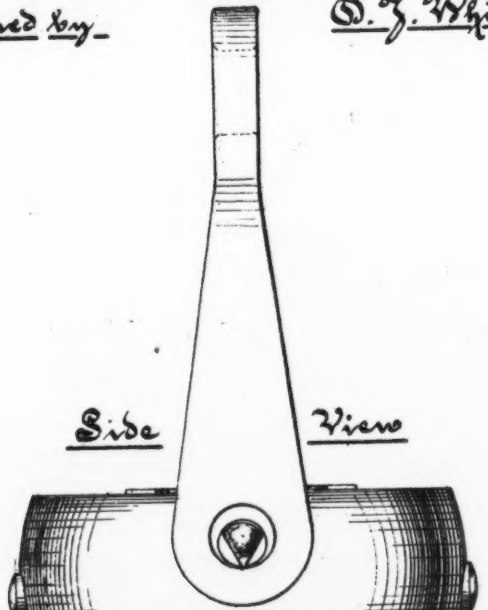
Briquette and Gimbal Clutch for Ascertaining Tensile Strength of Cements

Full Size.

Designed by D. J. Whittemore, C.E.

Bottom View of Gemel-Rings.

Side View

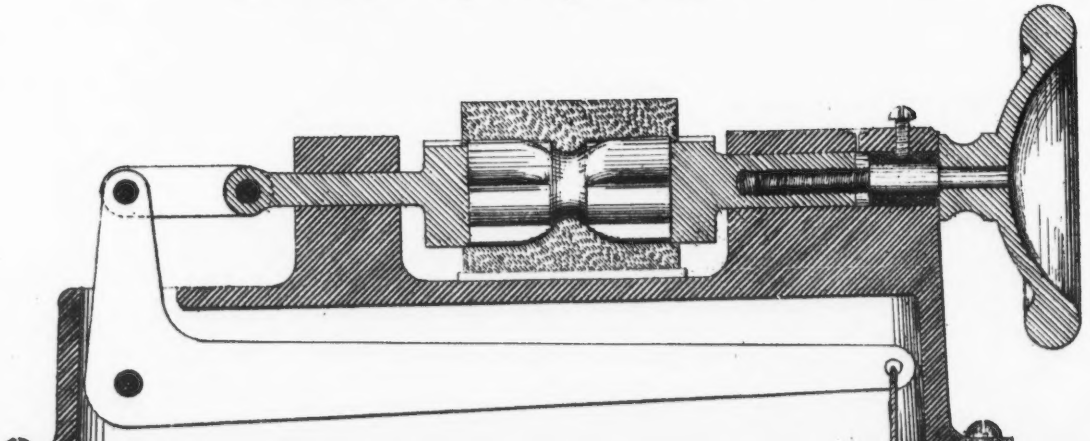


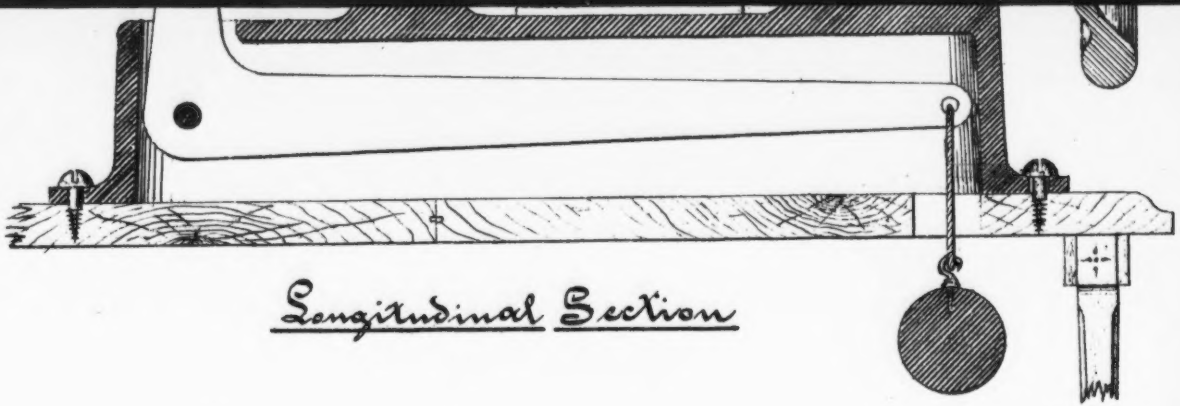
Press and Flask

for

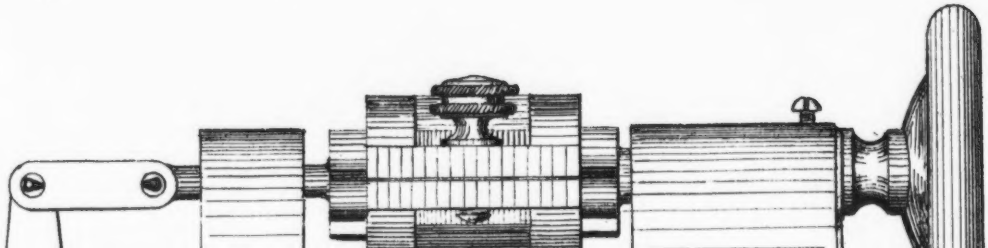
Molding Briquettes

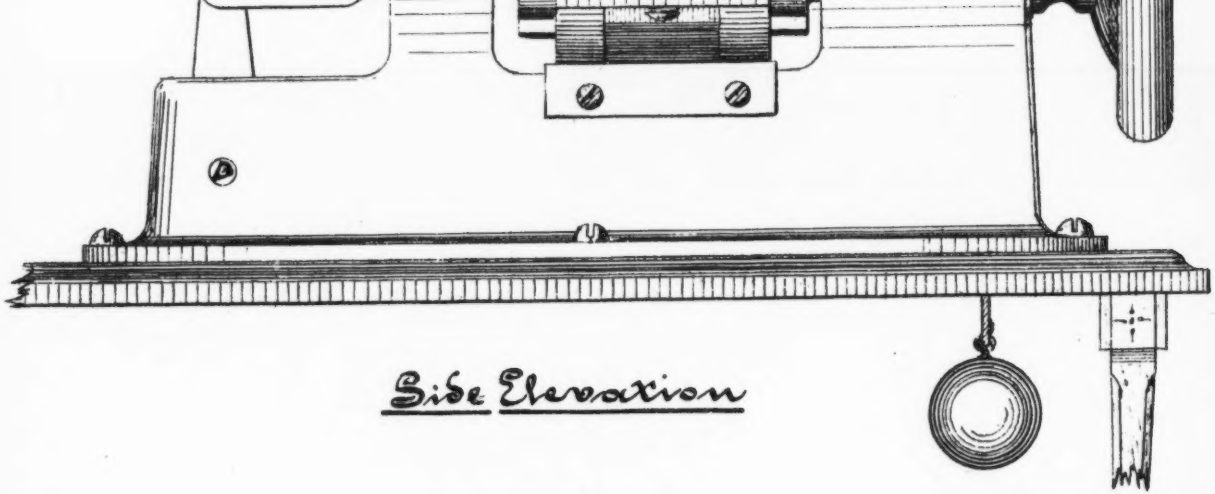
Designed by O. J. Whittemore, C.E.





Longitudinal Section





Side Elevation

Scale = Half Size

PLATE XVIII.
TRANS. AM. SOC. CIV. ENG'R'S.
VOL. LX. No. CCIII.
WHITTEMORE ON TENSILE TESTS OF CEMENT.

article, so finally we had to reject it altogether. But I will say that we never had in Chicago a better cement than we obtained from that mixture as long as the two cements were sent in good quality.

WILLIAM R. HUTTON.—I have used very quick setting cement from the Potomac, a yellow cement. It set too quickly, and we mixed it with Rosendale, the object being to make it set more slowly, and we made an excellent cement. We had no tests, but it worked smoothly, and good results followed. In the one case we had a very quick setting cement, which set in three or four minutes, but mixed with the Rosendale of the ordinary quality that we had, it set in about half an hour, with very good result.

F. O. NORTON.—The Rosendale ledges or quarries are of different strata, and they vary from the quick setting cement like the Potomac to cement that takes an hour and a half to set. They vary all the way from 3 minutes in setting, to an hour and a half. There are some thirty different strata in the Rosendale formations, and I think from a combination of all these different strata we get the best results. The quarries are capable of producing almost any kind, either quick or slow setting cement, and it is found very economical to work them all together, and they are usually worked in that way. Ordinary quick setting or ordinary setting Rosendale cement, should set in a mixture with sand in about three-fourths of an hour. Neat, it will set in half that time. But from the first mixture, in general use, it takes about three-quarters of an hour for the cement mortar to fairly set.

D. J. WHITTEMORE—It is very satisfactory to know that cement of the excellence shown by Mr. Norton's statistics can be produced in this country. That this is so, I feel is largely due to his individual efforts.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

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TRANSACTIONS.

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CCIV.

Vol. IX— September, 1880.

WATER-PROOF COVERINGS.

By F. COLLINGWOOD, Member of the Society.

READ AT THE TWELFTH ANNUAL CONVENTION, MAY 25, 1880.

With discussion by EDWARD P. NORTH, and an additional note by F. COLLINGWOOD.

As a contribution to the general subject of the use of asphaltes, bitumens, &c., the writer would offer the following bit of experience:

As it is intended that the arches of the approaches of the East River Bridge shall be used for mercantile purposes, it became necessary to cover the concrete over the arches with some protecting substance, which should keep out all dampness from the masonry. On inquiry among persons engaged in such work, a great diversity of material was offered, and each urged as the best for the purpose.

It was accordingly decided to ask bids generally, each bid to be accompanied by samples for testing.

In deciding on the tests, the following requirements were had in view:

1*st.* That the covering should be impermeable to water at all temperatures, and

2*d.* That it should be permanent.

It is evident that there is little danger of failure at high temperatures, but at low temperatures, cracks may develop from contraction or shocks, allowing water to pass through. The tests on this point were, to make some wooden moulds with grooves about 2 feet long, with enlargements at the ends, so as to give a shape like an eye bar, and fill them with the mixture. These, with some flat plates of the material, were exposed to a freezing mixture for 48 hours. Some of the long pieces cracked under this test. After freezing, the plates were first tested with a loaded point for penetration, and then dropped a short distance to see whether they would break. In this way a fair idea of the respective hardness and brittleness was obtained, and also (by careful measurements), the relative contraction.

After this the specimens were exposed to heat, to see their behavior under a high temperature.

The next tests were for durability; and proceeded upon the assumption that in process of time, destruction must occur, if at all, in two ways:

1*st.* By the gradual evaporation at ordinary temperatures of the volatile oils contained in the water-proofing material, and which give plasticity to the mass, thus causing them to be crumbly and absorbent, as witnessed in coal tar roofs. The test for this was to weigh with delicate scales a small mass of the material, and expose it in a sand bath, to a temperature of 212° for 48 hours, noting the percentage of loss.

The *second* possible cause of failure, was considered to be absorption of water, which by freezing and thawing would eventually cause disintegration.

For this test, carefully weighed specimens were soaked in water for 48 hours, taken out, wiped carefully, and reweighed to determine increase in weight.

A third possible cause of deterioration would be looseness of structure, causing rapid abrasion under passing loads, but this would be determined by the second test above.

The various samples offered may be classified as follows:

1*st.* Alternate layers of coal tar and felt.

2*d.* A mixture of soapstone, crude petroleum and resin.

3*d*. Trinidad bitumen (refined) and coal tar, with an admixtnre of sand.

4*th*. Asphaltic mastics consisting of various mixtures of Val de Travers or Limmer asphaltes, with Trinidad bitumen, residuum oil, limestone, sand, &c.

5*th*. Trinidad bitumen, with paper or felt, in consecutive layers.

6*th*. Trinidad bitumen (refined), mixed with about 10 per cent. of residuum oil, put on in consecutive layers.

The tests showed that all of No. 1 were totally unreliable for permanent work.

No. 2. Is an admirable fire proof material for roofs. There was a loss of one-half per cent. by boiling, however, and the price was too great.

No. 3. Absorbed 3 per cent. of water, and was evidently poorly prepared.

No. 4. Of these the best samples were excellent, but were rather hard at low temperatures. Of the poorer ones (those most adulterated), some failed by cracking, showing brittleness, others by absorption, taking up as much as 4½ per cent. of water. The best were too expensive.

No. 5. Were decided against, on the ground that no paper layers could be made continuous, and there seemed no reason for inserting a foreign substance which has no protective power of its own.

No. 6. A mixture of 20 per cent. residuum oil and 80 per cent. Trinidad bitumen was found to be too soft. The residuum oil or petroleum wax, is that product of the distillation of petroleum which passes over at temperatures between 800° and 900° Fahr. Reducing the oil to 10 per cent. gave a mixture neither too soft when warm, nor too brittle when cold, entirely non-absorbent, and losing nothing when boiled. It was therefore decided that as the price was less than that of any other acceptable sample offered, the bid would be accepted, to put down three layers having a total average thickness of half an inch.

The lesson to be learned from these tests in respect to the use of such materials in road making is this, that it is possible to take a piece of material from a street (a very small piece will do if chemist's scales are used), and determine at once whether there is a reasonable probability of its being of a lasting character. All experience goes to show that compressed asphalte cannot be durable unless well compressed; and the absorption test ought to reveal the physical condition of any specimen as to porosity. A tensile test, by comparison with known mixtures,

would reveal something also as to adulteration of the mixture by an excess of grit or earthy material. One bidder put in peat for some purpose best known to himself. Of course, no person would recommend the mixture adopted as suitable for road making, it can only be used where covered from the sun's rays by an external covering of some thickness, for the express purpose of keeping out water.

This experience is given as one method,—in the present case a very successful one,—of deciding among a lot of bids having apparently few points in common on which to base a decision.

EDWARD P. NORTH.—The interesting experiments detailed by Mr. Collingwood, may be supplemented by a notice of the tensile strength of some samples of asphaltic mastic (No. 4, mentioned above), and of a bituminous mastic known as Filbert's vulcanite.

The asphaltic mastic was made from Val de Travers asphalte, melted in the ordinary refined Trinidad bitumen, with the addition of about 50 per cent. of clean silicious grit of the size of a pea.

The vulcanite is identical with that tried in Fifth avenue, between Twenty-fourth and Twenty-fifth streets, in August 1879, and is composed of 14½ gallons of refined Trinidad bitumen, 7 cubic feet of fine sand, both heated to 300° F., and 1 to 1½ gallons of a mixture of cement, limestone dust and sulphur, all mixed in a horizontal pug mill.

The test pieces were one inch square, and were broken on a cement testing machine that only registered to 400 pounds.

Four samples of the vulcanite were broken at 120, 140, 185 and 230 pounds. Five samples of the asphalt mastic were broken, one at 352 and one at 400 pounds, the other three stood 400 pounds for over a minute and broke with an added weight of from 20 to 50 pounds. All the test pieces were immersed for over an hour before breaking in ice-water; the temperature of the room was 85°.

The asphaltic mastic probably had more than twice the tensile strength of the vulcanite, and would be less liable to crack from changes of temperature.

This mastic, without the addition of grit, is largely used in France for covering arches, casemates and powder magazines, with very satisfactory results, as the mastic yields to the settlement of the masonry without cracking.

It may be doubted if the test proposed by Mr. Collingwood would be a valuable aid in selecting a paving material, as newly laid com-

pressed asphalte would not show as well under it as the mastic, yet experience has shown that it is much more durable than the latter for wheelways, so much so that the Limmer Company, of London, are replacing their mastic pavements with compressed Sicilian asphalte, and it is doubtful if asphaltic mastic will wear better than the best of the bituminous mastics as now laid, particularly under rapid traffic in cold weather, but the asphaltic mastic would be the best absorbent of the two.

F. Collingwood.—In continuation of the discussion on "waterproof coverings," I have a few additional facts to offer.

Our work, as far as it was concreted, was covered during cold weather with the mixture mentioned above, viz., refined Trinidad bitumen with 10 per cent. of residuum oil. It was then expected that the stone paving would be laid early in the spring, and a thick layer of sand was spread over it. Instead of this, owing to delays, the pavement has not yet been laid (July 24th, 1880), and the whole has been exposed to the hot weather of the past few months.

The temperature has, under the circumstances, been sufficiently high to cause the bitumen to rise in places through the sand, and to flow to a moderate extent.

This experience shows that it is necessary to attend to the upper limit of temperature as well as the lower. I am now inclined to think that the bitumen would be better without any oil. With the full thickness of paving and sand we expect to put above it, the temperature will probably not be high enough to disturb it. This is, no doubt, the reason for the introduction of felt and paper in its use over roofs and sidewalk vaults.

There is a practical difficulty in applying it to the inside of drains. When hot enough to insure thorough adhesion to the walls of the drain, the smoke is so thick that the workmen cannot see what they are doing. The soap solution would probably be the best application in such locations.

The asphaltic mastic without grit is unquestionably a good material, as a number of such samples when tested, showed no absorption.

This would have been selected in the case of the bridge work, had not the price been higher than that of the bitumen. It was not considered enough better to warrant paying the considerable difference there was in the bids.

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(Vol. IX.—September, 1880.)

DISCUSSION

By DE VOLSON WOOD, MEMBER OF THE SOCIETY, ON THE RESISTANCE OF BEAMS.*

The main object of this paper appears to be to set forth a new theory of the resistance of beams; or perhaps, more strictly speaking, to explain in a new way the discrepancy which exists between the common theory and the results of experiment. (See §§ 24–32 of Emery's paper.) In order to show that more theorizing is needed, that writer has given an abstract of what is known upon the subject, together with the views of certain theorists; and these show not only that a discrepancy exists, but also that theorists are not agreed as to its cause. A critical study of the problem has baffled the efforts of every student who has attempted its solution; and yet the only condition required by theorists, in order to establish a correct formula, is to know *the law of action* of the internal forces in the section of rupture. The determination of this law is, therefore, the direct object of our study. Within the elastic limits the law is sufficiently well known, but that limit must be greatly exceeded before rupture can take place, and beyond that limit the law is different for different materials, and is by no means consistent for any class of materials.

It seems natural to assume that the law of extensions and compressions would be the same in a beam, as in a prism of the same material subject to a direct pull or push. Mr. Emery has subjected this hy-

*Flexure and Transverse Resistance of Beams: C. E. Emery, Vol. VIII., p.149. June, 1879.

pothesis to the test of experiment, and the work done being important (and I think original), I wish to bring forward two of his results. Experiments were made upon of a specimen material like that in the beam, and a curve constructed to represent the relation between the stress and strain to the point bordering on rupture. This was called *the experimental curve.* In § 17 he finds in one case that the modulus of stress on a unit of outer fibres, when the actual experimental curve was assumed to express the law of action of the internal forces, should be 29 027 lbs., whereas the actual ultimate tensile strength of the metal (cast iron) was 15 458 lbs., or about one-half that found above.

In another case, given in § 19, the experimental curve hypothesis gave 17 941 lbs., while the ultimate actual strength of the metal (gun metal) was 25 647 lbs., or nearly 50 per cent. more than that given above.

These results were a surprise, for we had long felt confident that this mode of treating the problem would give results at least approximately correct. It is true, as shown by Mr. Emery, that it is difficult, if not impossible, to obtain the true experimental curve at the point bordering on rupture; also that an empyrical equation was used to represent the curve. The curve is also modified by the manner of making the experiments; for if determined by repeated pulls, whether accompanied by shocks or not, it would be very different from that determined by a steady, continuous pull. We are, however, confident that in a large range of experiments, many cases would be found which would not only agree more nearly with experiment, but would, in those cases, give better results than by the common theory; still, if the examples given by Mr. Emery are not exceptional in their character, it appears that, in many cases at least, the experimental curve hypothesis is, theoretically, but little, if any, better than the common theory, and must be abandoned as practically useless. Even if it gave satisfactory results, the difficulty of using it is so great, that it would not be employed in ordinary practice, but would be confined to theoretical investigations.

Having shown the failure of this hypothesis, Mr. Emery gives his new theory in the following words (§ 25): "It is suggested as highly probable, in fact, almost a necessary inference, that on account of the lateral adhesion of the particles, the strains of greater intensity near the top and bottom, and at the centre of the beam, are in part transferred to the lamina of less strain in the direction of the curve nearer the axis, so

that the fibres crossing the central section of maximum moment are under greater strain than their proportional distance from the axis, and the outer fibres thereby correspondingly relieved." The "curve" here referred to is the isodynamic curve (or curve of equal stress); but I am uncertain as to the precise meaning of the expression "in the direction of the curve," but there is no doubt as to the general meaning—that fibres nearer the centre sustain more than their proportional part.

In regard to this hypothesis we need only say, that, if true, *the law of its action* is not given, and hence, new formulas cannot be founded upon it, nor the existing formulas modified so as to conform with it. But aside from this, it appears to us that the action (if any) due to the form of the curves of equal stress is quite too small to account for the great discrepancy which is known to exist. Until the law of action is given, very little can be said in regard to the hypothesis.

But we will make a remark in regard to the isodynamic curves. Their equation is (see (60):

$$Mk^n = \frac{Yb_\circ h_\circ^2}{1+n}$$

in which M is the moment of external forces, k the ordinate to the curve from the neutral axis, $b_\circ$ the breadth of the beam, $h_\circ$ its depth from the neutral axis, and n a constant depending upon the equation of the experimented curve, which equation is $y = ax^n$. In the equation of the curve if $M = o$, $k = \infty$; and if $k = o$, $M = \infty$; hence the curves are asymptotic, the vertical, through the origin being one asymptote, and the axis of the beam the other. If the beam be supported at its ends and loaded at the middle, the moment of the load will be $\frac{1}{4} P x$ (P being the load and x the abscissa), and also if $n = 1$ as in the common theory, the equation of the curves becomes

$$Pxk = Yb_\circ h_\circ$$

and hence, they will be equilateral hyperbolas; the curves from the opposite ends meeting at an angle at the center of the beam.

Mr. Emery remarks (§ 27), that "from this it appears that the increased strains due to the diagonal action of the curves, may become so important that the fibres will part at one side of the section of maximum moment as is frequently reported." The questions naturally arise—if such is the result in one case, why not in all cases; and if not in all cases, what conditions determine when it will and when not break at that point? It will be observed that the lines of equal stress are de-

termined according to the common theory, but if the action above referred to is considerable relatively, the curves will thereby be relatively modified ; and thus their equation become unknown.

Again, it is not necessary to resort to the theory of isodynamic curves in order to show that the fibres are not strained proportionally to their distance from the neutral axis. The experimental curves show that after the elastic limit is passed, the elongations are greater in proportion to the pulling force—a fact known long before these results were plotted. Assuming that the stresses are proportional to the ordinates from the neutral axis within the elastic limits, it follows that when that limit is passed at the outer fibres, it will require less force to elongate them proportionally, and hence, for a given increase in the elongation, those not strained to the elastic limit will be subjected to a greater proportional stress. There is no reasonable doubt that this action actually takes place. Here then is one reason why the common theory does not give correct results.

Confirmation of his hypothesis is inferred from the fact that the isodynamic curves resemble the lines of stress in glass, as shown by polarized light (§ 27). But the resemblance is scarcely more striking than that between the same lines and the lines of maximum stress, first deduced by Airy, the Astronomer Royal of England, and published in the transactions of the Royal Society of London. (See also the writer's *Resistance of Materials*, 3d Ed. p. 239). The resemblance, however, is only accidental, for the stress which causes the polarization of light is a shearing stress. The experiment with polarized light confirms the theory entertained by most writers upon shearing stress—that it is greater near the neutral axis, and least (or nothing) at the elements most remote from it ; also, that there is a shear in the vicinity of the pressures—one at each end, and another at the middle. If a beam be supported at its ends, and loaded with equal weights at points equidistant from the ends, that portion of the beam between the loads will be acted upon by a statical couple, and, according to theory, will have no shearing stress. And, generally, any portion of a beam acted upon by a statical couple, produced by any system of loading, will be free of shearing stress. In this case, also, the isodynamic curves will be parallel to the neutral axis. Here is a field in which experiments have not been made, and one which offers an opportunity for testing certain hypotheses in the resistance of materials. Thus, by means of polarized

light, the existence of a shearing stress in certain solids can be determined. If none exists, it can next be ascertained whether the absence of *shear* affects the formula for the strength of beams. According to theory, a beam, in such a condition, is equally liable to break at any section between the equal loads, and if it should break a greater number of times at one point than another, a search for the cause might result in a modification of theory.

In § 32 it is stated: "Evidently for solid beams the equalization of strain referred to must commence as soon as flexure begins, which explains the fact that the ordinary formulæ are incorrect, even within the limits of elasticity." This statement does not appear so evident to us. Whatever be the direction of action of the forces at the several points in the section of rupture, they can all be resolved into horizontal and transverse components; the former of which tend directly to pull or push the elements, and the latter, to pull or push them transversely. The former gives rise to the ordinary formula, and the latter to a modification of that formula on account of the deflection induced by the shear. Beyond this we see no ground for equalization within the elastic limits; unless we consider the infinitesimal change due to a change of form of the transverse section, a quantity far too small to be measured directly.

It will be observed that the form of the isodynamic curves is independent of the character of the material; but, if there is anything important in Mr. Emery's hypothesis, the action, due to their existence, must be greatly modified by such character. Thus, in many kinds of timber, the fibres are so well defined that a transverse action has a very different effect from a direct pull, and the same is true, though in a much less degree, with wrought iron; but cast iron is so nearly granular (or amorphous), that a force would produce nearly the same effect in any direction; and yet, strange as it may appear, in the last named substance, in which the elements are the most free to adjust themselves, the greatest discrepancy exists between theory and practice, in the case of beams.

Barlow's theory, combined with the common theory, gave results which agreed more nearly with experiment than any theory heretofore given; but its principles, as stated by its author, are so much at variance with those held by the most eminent writers that it had to be abandoned. For instance, Barlow stated that the longitudinal shear was caused by the unequal extension (or compression) of the consecutive fibres at dif-

ferent distances from the neutral axis, hence existed in all cases where a beam was bent; but according to the theory of Lamé, it is due to the *unequal* stress at two points of the *same* fibre. This view is supported by the most eminent writers since his day, including such names as Louiville, Weisbach and Rankine; and I think that this view is incontrovertible. According to Barlow's principle, a shear would exist in that part of a beam subjected to a statical couple, while, according to most other writers, no shear exists there. Again, Barlow stated that the shear would be uniformly distributed over the transverse section, because the extension of the fibres increases as their distance from the neutral axis; but, according to the theory of the writers above named, the shear is zero at the outer elements, and increases to the neutral axis, where it is greatest, a result which is confirmed, as before stated, by experiments upon glass with polarized light.

The breaking of materials is, physically speaking, a harsh operation. The materials used in constructions are comparatively gross, and rarely, if ever, break twice alike. This is illustrated by the pulling asunder of cylinders of iron and of steel. If the specimens be made as nearly alike as possible and from the same stock, they will exhibit different tensile strengths, but, fortunately, for such specimens, the difference between the greatest and least values of their absolute strengths is generally only a small fraction of the mean value of their strength. But this lack of uniformity in absolute strengths renders it impossible to make a formula which will agree *exactly* with practice in special cases. Still we do desire a theory which will give results approximately correct.

Just here we will indicate a cause of the discrepancy which has not heretofore, that we are aware of, been considered. Theory assumes that breakage takes place in a plane section, but this condition is never realized in practice. Is it not probable that if the entire surface in the section of rupture and the actual position of the neutral axis at the instant of rupture be used in the ordinary formula, the result would agree more nearly with experiment? Still further, if the "experimental curve" of extensions and compressions be also involved, would not the conditions give results which would agree as nearly with experiment as could reasonably be expected? Should it be found that these conditions would account for the recognized discrepancy, it would be of but little practical use, for one of the conditions, if not two of them, could not be accurately determined until after the beam was broken, and

the other would require an independent experiment with each piece to determine the experimental curve; still it would serve one good purpose—that of showing where the cause of the discrepancy lies, and thus remove the grounds for further theorizing on this point. Several considerations, founded upon past experience, may be made to bear upon this view of the subject at once. Thus, timber, for instance, in the ruptured section presents a splintered form, and in most cases it would be practically impossible to determine the actual area of rupture. In some cases wrought iron exhibits very clearly its fibrous character, not so much in the splintered appearance as in its separating in layers. In these kinds of materials, when the extreme fibres rupture at a point other than that at that of the maximum moment, may there not be a shearing between these fibres and those next to them not yet broken, which will aid materially in sustaining the load. In the rupture of cast iron and of steel there is no fibrous appearance, and it is not so plain how longitudinal shearing can act to resist breaking. If the principle which I have suggested above—that of considering the entire section of rupture—be the correct one to use in practice, it would seem, at first sight, as if fibrous bodies ought to give a greater discrepancy between theory and experiment than granular ones; but we are unable to say what accurate measurements would show. In the case of timber, if any part of the area of rupture consisted of the surface between fibres, that part should not be entered in the ordinary formula, for the stress on that surface would be a shear, and the modulus for shearing in timber is very small compared with the tenacity, in some cases being as low as $\frac{1}{10}$th the tenacity.

These suggestions show that there is still a field open for experiment, and until every question which is raised in regard to the rupture of beams is investigated, both theoretically and practically, should we despair of accounting, in a satisfactory manner, for the present discrepancy between theory and experiment.

This seems to be an appropriate place for giving a summary of what is supposed to be known in regard to this subject.

In a prismatic beam fixed horizontally at one end and broken by a load acting vertically at the free end, or such a beam supported at its ends on horizontal props and broken by a load at the middle point, we are supposed to know the following facts in regard to the action of the internal forces :

1. On the convex side the longitudinal elements (called fibres) are extended, and on the concave side compressed.

2. There are elements within the beam which are neither extended nor compressed, and the surface which contains these elements is called *the neutral surface.* Theoretically, this surface is assumed to be cylindrical, the elements of which are transverse to the beam; but in the solid it is probably very irregular, due to the non-homogeneous character of the material. This surface is not free from stress, for if it were, the same action would be secured by two beams of the same breadth and length but of half the depth, placed one upon the other; but it is well known that the action will be very different from what it would be with two such beams. The stress on the neutral surface is called *a shear.*

3. Whatever be the law of action of the stresses, the sum of all the compressive forces upon the fibres will equal the sum of all the pulling forces. This is merely putting in formal language one of the principles of Statics—that the sum of the axial components of all the forces will be zero for equilibrium. Since the forces act normal to the axis of the beam, they will have no component parallel to that axis, hence the algebraic sum of the stresses must be zero. This principle was correctly stated by Coulomb more than a century ago, but more recently two eminent writers, Navier and Barlow, both for a time used instead thereof the erroneous principle that the sum of the moments of the stresses in reference to the neutral axis was equal to zero. The principle enables one to determine the position of the neutral axis when the law of the stresses is known; and if the extensions and compressions are proportional to the stresses the neutral axis will pass through the centre of gravity of the sections, as shown by Navier about the year 1824. It also follows that if this law changes, as it does after the elastic limit is passed, the neutral axis may change, and in practice will move towards the compressed, or concave, side. The principle here stated is also very nearly true if the section of rupture be irregular, and it would be exactly true if the fibres were horizontal at the points of rupture, and the moment of the longitudinal shear be disregarded.

4. Whatever be the law of action of the forces for the beams here under consideration, the sum of the moments of the stresses on either side of the section of rupture will equal the sum of the moments of the breaking forces on the same side of the section. This is true, whether the section of rupture be plain or irregular; for it is simply a

formal statement of one of the general formulas of Statics that the sum of the moments of all the forces acting upon a body is zero for equilibrium. It is also independent of the position of the neutral axis. The difficulty of applying this principle correctly in practice consists in determining the stresses at different points of the section of rupture. They depend for their value upon the position of the neutral axis and their law of action.

5. There is a shearing stress at all parts of a beam not acted upon by a statical couple. This is shown by theory, and confirmed by experiment. There is, then, in the beams which we are considering, a shearing stress in all the transverse sections from end to end. A simple resolution of forces shows that there will be a transverse shear equal to the algebraic sum of the external forces between the section to the end—and, hence, in the beams we are considering equal to the forces at the end of the beam; and, according to the theory of shearing stresses, the longitudinal shear at every point of the beam will equal the transverse shearing at the same point. This is true for amorphous bodies, but the law may be modified for fibrous and heterogenous substances. The law of distribution of this stress remains to be determined; but for rectangular beams it is comparatively simple, being nothing at the outer elements on the concave and convex sides, and increasing at a uniform rate to the axis, where it is a maximum. On account of this stress the transverse sections of perfectly amorphous bodies will not remain plane during flexure, even within the elastic limits; but the deviation therefrom due to this cause will be so slight that it could be detected in an experiment only by extremely delicate measurements. The effect of the transverse shearing on the deflection of a beam can be computed, but the effect of the longitudinal shearing upon the ultimate strength is not definitely known.

6. If T be the modulus of tenacity of the material as determined by a direct pull, C the modulus of crushing determined by a direct push, R the modulus of rupture as determined from the ordinary formula ($\frac{1}{4} P l = \frac{1}{6} R b d^2$ for a beam supported at its ends and loaded at the middle), then it is found that R does not equal T nor C, but differs so much from both that we infer that there must be an error in the theory upon which the ordinary formula is founded. In nearly every case if $T > C$ then $R > C$ and $< T$, and if $T < C$, then $R < C$ and T; or in other words, the value of R is between those of T and C for the same material.

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DISCUSSION

BY ROBERT BRIGGS, MEMBER OF THE SOCIETY, ON THE VARIATION DUE TO ORTHOGONAL STRAINS IN THE ELASTIC LIMIT IN METALS.*

Mr. Robert Briggs remarked, that while it was impossible to discuss, with any completeness, so thoughtful and elaborate a paper as has just been presented, he still felt able to add some comments bearing upon unnoticed properties of orthogonal strains upon metals, or rather upon wrought-iron. As long since as 1856 to '59, he had become conversant with many facts upon this subject.

At the United States Capitol and Washington Aqueduct, then under the charge of Lieut. (now Maj.-Gen.) Montgomery C. Meigs, as an assistant-engineer, the speaker was an observer and participator in experiments upon the strength of materials to be employed on those works. By the tests of iron then instituted, it was demonstrated that when iron was subjected to gradual and continuous strain under loads exceeding the elastic limit, which for merchantable iron was generally at about 20 000 pounds per square inch, the ultimate strength or point of rupture under tension became elevated materially.

Wrought-iron of average quality, for bars, beams or plates, in directions of the rolling, was found, under progressive tests, when the whole time of effecting the ultimate test was only a few minutes, or such time as was needed to observe the extensions under the increasing strains, to have an average value of from 52 000 to 55 000 pounds per square inch. In such tests the ultimate extension under each load was rarely attained, but there would occur a large elongation nearly at once when any load exceeding the elastic limits and the previous load was applied, after

* Paper CXCI, by ROBERT H. THURSTON. Vol. IX. May, 1880.

which the bar would become apparently at rest. A portion of this elongation was permanent when the excessive tension was relieved and such portion was found to vary in some regard with the length of time during which the test piece had been permitted to rest under subjection to the load, *and the ultimate extension under that load was more closely attained.* It was also definitely established that when each increment of excessive strain (using the words — excessive strain, to express such strains as would produce permanent elongation), was allowed time to produce its effect in maximum *temporary* elongation, and when these strains, thus slowly applied, were carried to the point of rupture, a whole day, or even two or three days, being spent in testing the piece, the ultimate strength would rise to 60 000, or even 70 000 pounds per square inch for iron whose normal (so to speak) ultimate strength would scarcely exceed 50 000 pounds.

The experiments seemed to indicate with much positiveness, that any test which passed the primitive limits of elasticity, materially impaired the strength of the iron to resist a suddenly applied force, or a blow, but unfortunately they were not pursued as fully in this direction of investigation as they should have been. But they went far enough to impress the speaker with a belief in the hypothesis, which belief he has frequently affirmed since that time, that any impairment of the primitive elasticity of iron was a serious impairment of its serviceable strength in rendering it brittle. It is well known to all engineers, that chains, hooks and bolts, in ordinary service, are worked up to the strains of 20 000 pounds per square inch, or to the elastic limit of ordinary iron, and also that especially in the case of chains and hooks, this limit is surpassed frequently, and attendant upon such strains, repeatedly exercised, they become very short and brittle, and numerous instances of similar effect can be traced to a similar cause in general construction in iron. This view would make it appear that the increase of ultimate strength proceeding from an excessive straining elongation of iron bars is illusory, not to say dangerous.

That the experiment of Prof. Thurston should be followed out to the further consideration of possible orthogonal impairment of the material (and the vibrations occasioned by a blow or shock, are decidedly orthogonal stresses) is certain. In point of fact, it may be stated that the ultimate tensile value of iron is somewhere above 110 000 pounds per square inch, and that in accepting lower rates we are simply averaging imperfec-

tions or want of homogeneity in the particular sample under test. The formation of a fibrous condition is a severance of molecular continuity, so far as lateral cohesion is concerned, while it may be the means of bringing into action more numerous particles than would be in direction of the strain, in a bar wherein the crystals were merely entangled in all directions, or wherein only a portion of the perfect crystals have been extended as aminæ, or as filaments, in the one direction. The stiffening or strengthening of cold rolled shafting, or of drawn wire, would seem to conform to this hypothesis, while the notable increase of brittleness from these operations, is in a degree confirmatory to it. In further experiments in test beyond the limit of elasticity, it would be well to observe whether the specific gravity of the test piece were effected. It is probable that in the readjustments of the particles whereby new strength is added to the material, that the elongation is not attended with proportionate reduction of section. It is believed that this results in hard drawn wire—that it is less dense than soft annealed iron.

One other set of experiments were made at Washington, in 1858, having a bearing on this subject. In an examination into the strength of bolts, it became desirable to test the strength of iron under the combined strains of tension and torsion. Test pieces were placed in the testing machine, which were square in section of the body, and had round collars turned upon them for the places of rupture. By means of the squares there were attached wrenches, to the arms of which weights were applied at the same time that definite tensile strains were brought to bear on the test pieces. The result of these experiments was very positive, that no weakening of iron for tension occurred so long as the torsional strain did not exceed the elastic limit, and that no weakening of iron for torsion occurred so long as the tensional force applied did not pass the same limit, and that the impairment of strength to resist one strain when the other surpassed its limit, was too discordant to follow any law, although some indications showed that the component of the two strains might be assumed between the elastic limit and half the ultimate strength.

These experiments and their results remain with the *archives of Government;* as the charge of the Capitol construction and of the Washington aqueduct passed from the hands of General Meigs before completion, and subsequent engineers failed to come to an appreciation of their value.